the olive been tried? We have nowhere seen any account of such an attempt. One would suppose that it would grow well, and in that case could not fail to be very remunerative. The people are well-to-do, and the rate of wages is good. When one, however, compares what is done here and in the United States in the way of irrigation works, in the scientific investigation of the country with reference to mining and agricultural pursuits, and in the collection and examination of the objects of scientific interest, one cannot but feel that there is a sad lack of enterprise and energy in the colony. The Cape Town Museum seems to be in a semi-starved condition.

The white population of Natal is almost entirely English, the Dutch having withdrawn for the most part as soon as the English Government decided on interfering. Sugar seems likely to form the staple of the colony. It is cultivated with the aid of coolie labour, although the Zulus are to the white population as sixteen to one.

In the Transvaal and the Orange Free State the Dutch form the agricultural, the English the town and trading population. Mr. Trollope seems to possess that genial disposition which draws out the bright side of the people with whom he is brought in contact. Although, therefore, he finds the Boer wanting in cleanliness, education, sociability, and enterprise, he finds in him many good points, and is far from thinking him so bad or so hopeless as the author of "The Great Thirst Land." The Boer has improved of late years, and in some cases considerable pains are taken with the education of the children. As Mr. Trollope says, "The Dutch Boer is what he is, not because he is Dutch or because he is a Boer, but because circumstances have isolated him."

Three chapters are devoted to the diamond diggings, and a very interesting plan of the great Colesberg Kopje is given. The author has very little sympathy with diamond-digging, and the only satisfaction he finds there is the civilising influence which the employment of so many natives cannot fail in time to exert. Mr. Trollope has devoted considerable thought and attention to the native question. His opinion is one well worthy of attention, though it is not likely, he thinks, to be regarded with favour either by Exeter Hall or the Colonists whose lands lie uncultivated for want of labour. He visited several of the Missionary Institutions, all of which, with the exception of M. Esselin's self-supporting one at Worcester, seem to have been more or less failures. He thinks that work, steady and regular but voluntary, will be found to be the best and most effective civilising agents. Unfortunately the natives' wants are so few and so easily satisfied, that there is at present no spur to regular work.

The account of Bloemfontein as a sanatorium for consumptive people is that of a man of "heroic mould" equal to the feat of dining twice daily, such as Mr. Trollope must be, seeing that at his age he makes light of, and seems to have enjoyed, the rough travelling by mail-carts, cape-carts, and otherwise, of considerably over two thousand miles. One regrets that he has not mentioned whether there is here the same change between morning, midday, and evening climate as he observed at Pretoria; also whether he came across any consumptive people, and how they fared. He also forgets that deal benches and chairs constructed with an equal regard to human anatomy, judging from the fact that easy chairs cost 13*l.* 10*s.* each, are not the seats most likely to conduce to the comfort of an invalid.

An excellent map accompanies the book. The type, paper, and "get-up" are all that can be desired, and the number of misprints is small. W. J. L.

OUR BOOK SHELF

The Science of Language. By Abel Hovelacque. Translated by A. H. Keane. (Chapman and Hall, 1877.)

We have already had occasion to review at length the original French text of this work, which is now presented in an English dress. M. Hovelacque is one of the most distinguished representatives of the school of comparative philologists who would include their study among the physical sciences, and his book illustrates both the faults and the excellences of the view he upholds. In spite of the limitations thus introduced into the science of language, in spite, too, of the many inaccuracies which occur in his descriptions of the various groups of language at present existing in the world, the clearness and vigour of his style make his book one well worth translating, and it is satisfactory to see that it has been put into competent hands. Mr. Keane has added to the value of the work by a philological map, and a tabulated list of the languages described by M. Hovelacque, together with their characteristics and geographical position. From time to time, too, he has introduced foot-notes and even insertions in the text; many of these give fresh information or correct the statements of the author; others of them, however, had better been left unwritten. Thus his reference to Raabe's attempt to connect Aryan and Semitic grammar is not very happy, and he is unfair towards his author when he accuses him of inconsistency in being at once a Darwinian and a polygenist. No doubt "the impossibility of reducing man now to, say a mollusc, is no argument against the original identity of man with a mollusc" (or rather of his descent from the same form of life as a mollusc); but that is because there are intermediate links and stages of development between the mollusc and man, and M. Hovelacque believes—and with good reason—that such intermediate links do not exist between the manifold families of speech that are scattered over the world.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

Age of the Sun in Relation to Evolution

It has been urged by Mr. Plummer (pp. 303 and 360) as a fundamental objection to the theory that sun-stars are formed from the collision of stellar masses, that if the theory be true there ought to be many of the stars moving with great velocities, which he affirms is not the case. But I am unable to understand upon what grounds he bases his assertion. I freely admit that if it could be proved that none of the stars has, as he seems to suppose, a proper motion of more than thirty or forty miles per second, it would at least be a formidable difficulty in the way of accepting the theory. For it would indeed be strange, as Mr. Plummer remarks, "that amid all the diversity of dimensions of the heavenly bodies, it should invariably happen that the resultant movement of the combined masses should be reduced to such insignificant figures as the above." But how does Mr. Plummer arrive at the conclusion that something like this must invariably

have taken place? I fear that before his objection can be fairly urged something more definite must yet be known as to the rate of motion of the stars.

All that we are at present warranted to affirm, I presume, is simply that of the comparatively few stars whose rate of motion has been properly measured, none has a motion greater than thirty or forty miles per second, while nothing whatever is known with certainty as to the rate of motion of the greater numbers of stars. Before we can ascertain the rate of motion of a star from its angular displacement of position in a given time we must know its absolute distance. But it is only of the few stars which show a well-marked parallax that we can estimate the distance, for it is now generally admitted that there is no relation between the apparent magnitude and the real distance of a star. All that we know in regard to the distances of the greater mass of the stars is little more than mere conjecture. Even supposing we knew the absolute distance of a star and could measure its amount of displacement in a given time, still we could not be certain of its rate of motion unless we knew that it was moving directly at right-angles to the line of vision, and not at the same time receding or advancing towards us; and this we could not determine by mere observation. The rate of motion, as determined from its observed change of position, may be, say, only twenty miles a second, while its actual velocity may be ten times that amount.

By spectrum analysis it is true we can determine the rate at which a star may be advancing or receding along the line of sight independently of any knowledge of its distance. But this again does not give us the actual rate of motion unless we are certain that it is moving directly to or from us. If it is at the same time moving transversely to the observer, its actual motion may be more than 100 miles per second, while the rate at which it is receding or advancing, as determined by spectrum analysis, may not be twenty miles a second. But in many cases it would be difficult to ascertain whether the star had a transverse motion or not. A star, for example, 1,000 times more remote than α Centauri, that is, twenty thousand billion miles, though moving transversely to the observer at the enormous rate of 100 miles per second, would take upwards of thirty years to change its position so much as 1″ and 1,800 years to change its position 1′. In fact, we should have to watch the star for a generation or two before we could be certain whether it was changing its position or not. And even after we had found with certainty that the star was shifting, and this at the rate of 1′ in 1,800 years, we could not, without a knowledge of its distance, express the angle of displacement in miles. But from the apparent magnitude or brilliancy of the star we could not determine whether its distance was ten times, 100 times, or 1,000 times that of α Centauri and consequently we could form no conjecture as to the actual velocity of the star. If we assumed its distance to be ten times that of α Centauri, this would give a transverse velocity of one mile per second. If we assumed its distance to be 100 times that of α Centauri, this would give ten miles a second as the velocity, and if 1,000 times, the velocity of course would be 100 miles per second.

As there are but few of the stars which show a measurable parallax and having no other reliable method of estimating their distances, it follows that in reference to the greater number of the stars neither by spectrum analysis nor by observation of their change of position can we determine their velocities. There does not therefore appear to be the shadow of a reason for believing that none of the stars has a motion of over thirty or forty miles per second. For anything that at present is known to the contrary, the majority of them may possess a proper motion enormously greater than that.

There is, however, an important point which seems to be overlooked in Mr. Plummer's objection, viz., that unless the greater part of the motion of translation be transformed into heat, the chances are that no sun-star will be formed. It is necessary to the formation of a sun which is to endure for millions of years, and to form the centre of a planetary system like our own that the masses coming into collision should be converted into an incandescent nebulous mass. But the greater the amount of motion left unconverted into heat, the less is the chance of this condition being attained. A concussion which would leave the greater part of the motion of translation untransformed would be likely as a general rule to produce merely a temporary star, which would blaze forth for a few years or a few hundred years, or perhaps a few thousand years, and then die out. In fact we have had several good examples of such since the time of Hipparchus. Now, although it may be true that according to the law of chances, collisions producing temporary stars may be far more numerous than those resulting in the formation of permanent stars, nevertheless the number of those temporary stars observable in the heavens may be perfectly insignificant in comparison to the number of permanent stars. Suppose there were as many as one hundred temporary stars formed for one permanent, and that on an average each should continue visible for 1,000 years, there would not at the present moment be over half-a-dozen of such stars visible in the heavens.

JAMES CROLL

The Age of the Earth

WITH reference to the ingenious suggestion by Mr. Preston, on the earth's orbit having been practically diminished by ethereal retardation, there are a few other points to be considered. 1. That the minor planets could never have passed the major planets, as they would be certainly caught by them during the immense number of revolutions in which their orbits would be nearly equal. Therefore the earth cannot have dropped in from much farther than Jupiter's present orbit; for if during its revolutions it came within one-sixth of the distance from Jupiter that it now is from the sun, it would be mastered by Jupiter. 2. By the retardation of Encke's comet it seems that if the comet had the same orbit as the earth, its distance from the sun would diminish about $\frac{1}{2\,5\,0\,0}$ per year. But for any appreciable lengthening of the earth's life-period, the earth must have started much more than one-tenth farther from the sun than it now is; that is to say, it must fall in much quicker than at the rate of its present distance from the sun in 10^8 years. This shows that the individual portions of Encke's comet must be much more than two miles in diameter, even supposing it to have as great a mean density as the earth, and to consist of a shower of solid meteors. Thus if the earth's history should be lengthened by any important amount from this cause, the nucleus of Encke's comet must consist of a shower of bodies of as great a density as the earth, and of a considerable size, each weighing very much more than 100,000,000 tons. And considering that there must be thousands of such bodies to compose it, the total mass would be greatly beyond what is considered possible. 3. If the earth had drawn much nearer to the sun, the asteroids must have come in from a very much greater distance; and yet, though they differ greatly in size, they are all grouped closely together, whereas we should find then sorted out very much more widely, and a vast quantity of them retained by Jupiter as satellites.

The solar system appears to be really a quinary system of stars; the major planets being analogous to the sun in their characteristics of density, distances and proportions of satellites, and other elements, the minor planets being the sun's satellites. Thus it is seen that the uniform law of satellites is to regularly decrease in volume both close to, and farthest from, their primaries; the series manifestly terminating in asteroids in the case of the sun and of Saturn.

In the whole of the present discussion of the earth's age, what is the reason why only one out of several different limits is considered? 1. The decrease of temperature in the earth. 2. Tidal retardation. 3. The cooling of the sun, which is recognised as being the weakest of the three. 4. A uniform diffusion of temperature in the earth, which gives a limit, not for life, but for the separate existence of the earth. The close agreement of the limits of life history given by these first three methods is a very strong argument in favour of each of them; for if there is even a possibility of 1 in 5 that each separately is wrong, it would be less chance than 1 in 100 that the concordance of all three was wrong.

Is there anything so stable and certain in geologic time—when we remember that levels permanently alter as quickly as ten feet per century—that rainfall (and therefore denudation) depends mainly on the almost unknown changes in the sun's heat, a slight increase of rainfall making much greater rapidity of denudation—and that accumulation of peat and stalactite might well become proverbial for its variability—when all these uncertainties are remembered, is there anything so indubitable as to warrant our throwing all the odium of incorrectness on the cosmical chronology, and seeking to square it with geological suppositions?

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